Center for Independent Experts (CIE) Peer Review

of the

Draft Recovery Plan

for

California's North Central Coast Domain Chinook and Steelhead

John G. Williams
December 2011

29665 Mattole Road, Petrolia, CA
Mail to: PO Box 214, Petrolia, CA 95558
707 629 3265 jgwill@frontiernet.net

Executive Summary

This is a scientific peer review of the Draft Recovery Plan (DRP) for Chinook and steelhead in the California North Central Coast Domain Draft. The DRP comprises three volumes, with almost 2,000 pages. Volume 1 gives the meat of it for the domain. Volume 2 presents descriptions of individual streams as well as tables of stream-specific recommendations for recovery actions, but it is incomplete, and lacks profiles for many streams in the northern part of the domain. Volume 3 provides important background material.

Volume 1 is based largely upon good documents produced by the Technical Recovery Team (TRT) for the domain. However, it does not account for important studies on the negative effects of hatchery fish on naturally reproducing ones, and on steelhead, that have been published since the TRT completed its work (probably in 2007). Taking this new information into account should change some of the assessments and recommendations in the DRP. Although Volume 1 includes a chapter on climate change, it does not adequately integrate this into the rest of the document. Accordingly, the DRP is not based on the best available science.

There are additional substantive problems with the DRP. Volume 1 identifies the mixed-stock ocean Chinook fishery as a source of mortality for California Coastal Chinook, but proposes no significant actions to address it. Volume 1 also depends too heavily on an approximate index of habitat, *IPkm*, and on the results of the Conservation Action Planning (CAP) assessment protocol; both of these are useful tools, but should be used in conjunction with thoughtful qualitative assessments of particular streams, such as those in the stream profiles in Volume 2. However, profiles are not provided for some streams, and the tables of site-specific actions for others are essentially blank. Additionally, some of the recovery actions for diversity strata were not finished in time to include in the CIE review draft, so the DRP does not provide the kind of recovery actions called for by Section 4(f)(1)(B) of the ESA.

Volume 1 was evidently prepared with undue haste. It appears that the chapters were prepared separately, and little if any effort was made to blend them into a coherent whole, so that the volume is highly redundant and sometimes self-contradictory. Several of the chapters are in great need of copy editing, some contain simple mistakes, and others have far too much jargon or organization-speak.

NMFS should allocate a good deal of time and effort to improving the DPR before it is sent out for a second round of review. In addition to addressing the deficiencies noted above, the next version should incorporate an adaptive approach, as suggested in the 2007 NMFS guidelines. The lack of data regarding Chinook and steelhead in the domain makes an adaptive approach particularly appropriate. The up-to-date monitoring plan proposed by the DPR could be an important element of such an approach.

I regret being so negative. In my opinion, many of the problems with the DRP result from the undue separation between the scientific and regulatory branches of NMFS, with the result that documents such as Volume 1 are prepared by staff whose jobs do not allow them to be practicing scientists, or even to take the time to keep properly abreast of scientific developments. That is, the shortcomings in the DRP result primarily from the organization of NMFS, rather than personal shortcomings of the authors.

Introduction

This is a scientific peer review for the CIE of the Draft Recovery Plan (DRP) for Chinook and steelhead in the California North Central Coast Domain Draft. The Statement of Work for the review, attached as an appendix, notes that: "CIE reviewers are contracted with the qualifications to conduct a scientific peer review, and are not required to provide regulatory or management advice." However, I do not read this as an instruction not to do so, so I have offered management advice where I thought it appropriate.

The DRP comprises three volumes, with almost 2,000 pages. Volume 1 gives the meat of it for the domain. Volume 2 presents descriptions of many but not all of the individual streams in the domain as well as tables of viability, stress and threat assessments, and a table of recommendations for recovery actions for each stream. The appendices in Volume 3 provide important background material such as Spence et al. (2008), or details regarding implementation of the plan, such as the Conservation Action Planning (CAP) method used for assessing habitats.

The DRP is likely to be controversial. According to the CIE email to potential reviewers:

> The NMFS Recovery Plan for the North Central Coast is expected to generate substantial interest from outside parties because it: (1) will contain recommendations involving water supplies for a variety of municipalities (including the greater San Francisco area) and agricultural users; (2) will prioritize watersheds for targeted restoration actions; (3) could influence local and regional planning efforts and decisions involving land development patterns such as county policies and forest practices; and (4) may advise state agencies and local governments on other actions necessary for recovery. The draft recovery plan will include a large geographic area in central California and has the potential for wide-ranging implications. Stakeholder interest will be high and likely lead to inquiries from elected representatives at the state and Federal levels.

Moreover, the success of the plaintiffs in the recent Wanger decisions, especially the 31 August 2011 decision on the consolidated Delta smelt cases, suggests that stakeholders whose interests are adversely affected by attempts to implement the Recovery Plan will challenge the underlying science, if they see weaknesses in it. Accordingly, it is important that the Recovery Plan be of particularly high quality, and be solidly grounded in the best available science. My review of the DRP takes this into account.

To conduct the review, I read the reports of the NCC Technical Recovery Team (TRT) and other background material specified in the Statement of Work, as well as other articles on important topics raised by the DRP. Much of this was familiar, since I was a member of the

Technical Recovery Team for Central Valley Chinook and steelhead, which produced similar reports. I read the material in Volume 3 and a sample of the stream profiles in Volume 2, especially those about which I have some knowledge, but focused my review on Volume 1. I am not an expert on structured assessment protocols such as the CAP, and spent less time on the detailed description of it in Vol. 3 than it deserves. This is a limitation of my review. Another limitation is that we were allotted eight days to review this long and complex document, so the review is not as complete or careful as I would like it to be.

I have cited my own publications rather frequently because this is an easy way for me to expand upon what I am writing in this review.

Initial Comments

I depart here from the outline suggested in the Statement of Work[1] to report my first finding, that the DPR was sent out for review prematurely. It appears that the individual chapters of Volume 1 were prepared separately, and then simply put together without any effort to make them a coherent whole. In consequence, Volume 1 is highly redundant and sometimes contradictory, which has made reviewing it much more difficult. For example, Volume 1 gives one set of "overarching goals" in Ch. 1, and a quite different set in Ch. 9: At p. 4:

> Overarching goals of the recovery plan are to:
> q Provide information on the life history of NC steelhead, CC Chinook salmon and CCC steelhead related to their endangerment and recovery;
> q Outline a transparent and adaptable strategy to achieve recovery;
> q Identify highest priorities, and recovery actions targeting those priorities;
> q Establish criteria to measure the achievement of recovery; and
> q Provide a framework for outreach, funding, and collaboration for recovery.

At p. 163:

> The overarching goals of this recovery plan are to prevent the extinction of these salmon and steelhead and ensure their long-term persistence towards a viable, self-sustaining, and eventually harvestable status.

As another example, at p. 65, it states: "The NC steelhead ESU historically consisted of 5 diversity strata with 41 Independent populations of winter-run steelhead (19 Functionally Independent and 22 potentially Independent) and as many as 10 populations of summer steelhead (all Functionally Independent) (Figure 9)." At p. 135: "The NC steelhead DPS historically supported 44 Independent populations of winter-run steelhead (21 Functionally Independent and 23 potentially Independent) and as many as 12 populations of summer steelhead."

[1] The Statement of Work is attached as an appendix.

As examples of a different sort, the last sentence in the first paragraph of Ch. 2 is "Additionally, unlike salmonids, steelhead are iteoparous (sic), meaning that not all adults die after spawning and, thus, some may spawn more than once." Clearly, the intended contrast is with the so-called Pacific Salmon. Similarly, Chinook do not have 10-14 dorsal fins; they have 10-14 rays in their dorsal fins. Presumably, these were just slips of the mind, but they illustrate the lack of care in preparing Volume 1. Several sections of the chapter on recovery actions have not been written. Additionally, several of the chapters are in severe need of copy editing, as shown by a recommendation at p. 155: "Assess imported water impacts to steelhead by conducting a detailed study assessing the effects of imported water on affected populations." Many paragraphs in some chapters lack topic sentences, which makes them hard to read. Many of the citations are incomplete, and seem to refer by number to some NMFS database; others are simply missing. The discussion of extinction and recovery trajectories in Ch. 4 borrows too heavily from one of the sources cited, Melbourne and Hastings (2008), to the point of copying several sentences essentially *verbatim*. Figure 12 is not mentioned in the text or adequately explained in the legend. IP-km is defined incorrectly in the glossary of Vol. 1. Citations are sometimes missing where they are needed, for example after the claim at p. 76 that "NC steelhead, CC Chinook salmon and CCC steelhead are all at high risk of extinction." There are substantive issues as well, discussed below, so my first recommendation is that NMFS allocate a good deal of time and effort to improving the DPR before it is sent out for a second round of review.

1.0 Evaluate the adequacy, appropriateness and application of data used in the Report.

With a few exceptions, data on Chinook and steelhead populations in the domain are sparse. The then available data were used in the two Technical Recovery Team (TRT) reports, Bjorksteadt et al. (2005) and Spence et al. (2008), and these are used in the DRP. Some important data developed in the last few years are not included, as discussed in the next section.

1.1 In general, does the Report include and cite the best scientific and commercial information available on the species and its habitats, including threats to the species and to its habitat including large-scale perturbations such as climate change and ocean conditions?

The DRP does not consistently make use of the best available scientific information. Volume 1 is largely based on two TRT reports mentioned above. These are good reports by competent scientists trying to make do in a data sparse situation. The reports generally do use the best information available at the time, are well written, and clearly describe the reasoning behind their conclusions and recommendations. However, information on Chinook and steelhead is accumulating very rapidly, and so important new information has become available since the work on the TRT reports was completed, and the DRP generally does not take account of it. Two clear examples involve regarding hatchery influence and steelhead.

Hatchery influence

Volume 1 depends heavily on Spence et al. (2008) regarding hatcheries, and does not consider recent, highly relevant publications (e.g., Araki et al. 2007;2008; 2009; Berejikian et al. 2009; Bernston et al. 2011; Buhle et al. 2009; Chilcote et al. 2011).[2] The viability criteria in Spence et al. (2008) include a row for "hatchery influence," but unlike several of the other TRTs, Spence et al. (2008) elected not to set quantitative criteria for low, medium, or high risk in terms of the percentage of hatchery fish among fish spawning naturally. Instead, they gave qualitative criteria, in terms of "evidence of adverse genetic, demographic, or ecological effects on wild populations." Spence et al. (2008:52), in discussing their population viability criteria, noted that "The proposed population viability criteria represent our best judgment given the available scientific information, and we fully acknowledge that these should be considered preliminary and subject to change if credible scientific evidence suggests that the criteria are inappropriate, either as general criteria or on a case-by-case basis as population-specific information becomes available." The articles listed above provide strong evidence for negative "genetic, demographic or ecological effects" of hatchery production on naturally produced fish (abstracts are attached as an appendix), so the qualitative criteria should be reconsidered in the DRP. Additionally, consideration of conservation hatcheries is a recurring recommendation in the Volume 1, and any such consideration should take this new information into account.

Steelhead

Volume 1of the DRP does not take adequate account of the ongoing research on steelhead at the NMFS Fisheries Ecology Division in Santa Cruz, and at the adjacent UCSC Long Marine Laboratory, which has produced a spate of papers. These were published after Spence et al. (2008) completed their report, so Spence et al. (2008) and Volume 1 cite Morgan Bond's thesis, and not (except in a section on Bay Area habitats) his subsequent publication (Bond et al. 2008). Other relevant papers from this group and colleagues include: Hayes et al. 2008, Mangel and Satterthwaite 2008; McClure et al. 2008; Sogard et al. 2009; Satterthwaite et al. 2009; Beakes et al. 2010; Moore et al. 2011; and Hayes et al. 2011. These papers provide important new information on the use of lagoons by juvenile steelhead, and new understanding of the biology underlying the diversity of steelhead life histories, with consequences for recovery efforts. The failure of the Volume 1 to take account of these papers underscores the lack of effective communication between the scientific and regulatory branches of NMFS, a point that I discuss further below. Werner et al. (2005), regarding thermal stress in steelhead in the Navarro River, is also not cited, although it is relevant to restoration and monitoring.

[2] I assume that the deliberations of the TRT were completed before Araki et al. (2007) was published.

Does it matter that the DRP does not cite the most recent papers on steelhead? I think it does, except for the coastal streams in San Mateo and Santa Cruz counties. For those streams, the profiles in Volume 2 do emphasize the importance of the estuaries, probably because agency biologists working in this area have been influenced by the largely unpublished work of Jerry Smith. In the profiles for streams around the bay, such as Sonoma Creek or the Napa River, or for coastal streams farther north, the importance of estuaries for steelhead gets much less attention than it should, in light of this new information.

Other substantive issues regarding the information used include the following:

Dated authorities

Chapter 2 of the DPR, "NCCC Salmonids," depends too heavily on dated sources such as Healey (1991), or on general works such as Moyle (2002), and neglects more recent reviews such as Quinn (2005), as well as the current literature. For example, a paragraph on the use of estuaries or lagoons by juvenile steelhead (at p. 15) cites one source from 1982 and one from 1990, but not the more recent and highly pertinent papers listed above. Not only is newer information neglected, but the text is simply incorrect about the juvenile life history of spring Chinook south of the Columbia River, many if not most of which are ocean-type (Ewing et al. 2001; Williams 2006). Similarly, it is not the case that the fecundity of Chinook is "not directly correlated to fish size" (p. 10); one of the citations on this point is to Moyle (2002), who says instead that the relationship is not as strong as in other salmon.

Climate change

Volume 1 includes a chapter on climate change and the associated sea-level rise, but the other chapters generally fail to integrate this material. For example, the threats assessment of Ch. 3 and the CAP protocol described in Vol. 3 take only cursory account of it, as do discussions of estuarine habitat in Volume 1. Accounting for climate change and the associated sea level rise is not easy, but certainly can be done. Cloern et al. (2011) provide a state-of-the-art example, but simpler methods could also be used. Modeling results for different climate change scenarios are available (e.g., Dettinger 2005), and a set of these could be used for more qualitative assessments. The viability table taken from Spence et al. (2008) deals with the risk of extinction over 100 years, which provides a logical time-frame for the assessments.

Conservation Action Planning:

Using some structured assessment protocol such as the Conservation Action Planning (CAP) process used for the DPR seems necessary, but I do not think it is sufficient. That is, the results of the assessment need to be considered carefully and critically, to check whether they make sense in light of what is known about the stream in question. In particular, are the tables at the end of the stream profiles in Vol. 2 consistent with what is said in the narrative? I am not expert in the use of these assessment protocols, as noted above, and have not had time to study this one carefully, but one way or another these protocols act as filters, boiling down a large number of considerations into one rating, and things are bound to be distorted or lost in the process. Worse, the conceptual models or hypotheses underlying the filtering are apt to get obscured, so that the filtering is not adjusted to account for new information, for example that coming from the recent papers on steelhead (see the recommendation below on adaptive management for another potential example). The CAP process is a powerful tool, but it is a potentially dangerous one, and should be used carefully.

Intrinsic Potential (IP)

I think the DRP depends too heavily on *IPkm* (or IP-km, or IP km, depending on the volume or chapter), an index of habitat length. *IPkm* was also used by Bjorksteadt et al. (2005) and Spence et al. (2008). One problem faced by Bjorksteadt et al. (2005) was to decide, when reliable historical data were lacking, whether the different streams in the region historically supported populations that were large enough not to depend on immigrants from other streams to persist. Briefly, they did this using a GIS-based model that estimated the suitability of lengths of stream from three physical variables, channel gradient, an index of valley width, and mean annual discharge, giving each a score between 0 and 1. They then assumed that the number of potential redds in each kilometer of a stream varied linearly from 0 for a suitability (IP score) of 0, to some maximum for an IP score of 1. Then, the IP scores of each kilometer of stream could be added together to produce a weighted sum for the stream. For Chinook, they assumed the maximum number of redds in a kilometer of good habitat to be 20. Then, they assumed that a breeding population of 2500 was needed, which, assuming equal numbers of males and females, and a four-year generation time, implies a need for 1,250/4 = 313 potential redd sites. Thus, they estimated the IPkm of available habitat needed to support an independent population as 313/20 = 15.6. Finally, to be conservative in their estimate of the total number of independent populations, they upped 15.6 to 20. Similar calculations were done for steelhead. Figure B1 in Spence et al. (2008) shows a reasonable relationship between *IPkm* and the estimated historical populations for nine streams, with an R^2 of 0.51.

For this purpose, *IPkm* seems useful and defensible. However, as should be clear from the discussion above, it is a blunt instrument, built from brave assumptions, and should be used with care and caution, especially when historical or current information is available. One way to think about the limitations of *IPkm*, or other indices based on physical habitat, is to consider the historical variation in the sockeye productivity of different rivers draining into Bristol Bay (Hilborn et al. 2003), for which gross measures of physical habitat such as gradient and valley width presumably have not changed. Annual discharge has varied over the period of record, but this raises a question about the appropriate period over which to average. Similarly, Hill et al. (2010) report that salmon populations are depressed in the pristine and protected Kitlope watershed in British Columbia, probably because mixed-stock fisheries have reduced the subsidy of marine-derived nutrients. Another limitation is that the habitat use by particular species depends upon the other species present; for example, juvenile steelhead may use lower gradient habitat when juvenile coho are scarce or absent. Productivity depends on biological as well as physical factors.

Spence et al. (2008) used *IPkm* somewhat differently, for example in the population viability criteria in their Table 1. I think this use is less satisfactory, because other information is likely to be available, and should be if it isn't. For example, there are no data on pre-disturbance particle size distributions in spawning areas in the rivers in the NCC Domain, but generally there are such data on streams now.

The DRP goes even farther than Spence et al. (2008), I think too far, in its use of IPkm. I do not see the point of setting targets of *IPkm*, even if *IPkm* is used to set abundance targets (suppose the abundance targets are met by natural production, but the *IPkm* targets are not, say because a population of steelhead is making heavy use of lagoon habitat). For selecting promising reaches of stream in which to focus recovery efforts, it can certainly be used as for suggestions about where to look, but it should not be used as a substitute for doing so. My assessment is that using *IPkm* as an approximate index is defensible, especially if it is used in concert with other information, but where better information is or could be available, it is a weak reed to lean on. It should not be used as a substitute for careful assessments of individual streams, such as those given in Vol. 2.

Another reason for not relying too heavily on *IPkm* as defined by Bjorksteadt et al (2005) is that using landscape attributes to estimate fish habitat seems to be an active area of work (e.g., Anlauf et al. 2011; Busch et al. 2011), so better indices may well be developed. Relying heavily on *IPkm* for the recovery plan would make changing to the improved index more difficult.

Apart from the substantive issues, the discussion of Intrinsic Potential in Vol. 1 is unnecessarily confusing, and I had to refer to Bjorkstead et al. (2005) to figure it out what it meant.

Stream profiles

Volume 2 comprises assessments of most of the streams in the NCC domain. Unfortunately, the volume does not cover many of the streams in the domain, particularly those in the northern part, and the information in the profiles does not seem to be integrated into Vol. 1. As a simple example, the profile on Scott Creek cites recent papers on steelhead noted above that Vol. 1 does not. There are minor inaccuracies in the profiles of the streams with which I am most familiar, e.g., Pescadero Creek, and I suggest ways that they could be improved below, but I think the basic approach in invaluable. Tension between generalities and particulars advances understanding, and these profiles provide the relevant particulars. The stream profiles also allow individuals with local knowledge assess and perhaps contribute to agency understanding.

Natural conditions presumption:

The DRP depends heavily on "… the operating hypothesis that populations, as they functioned in their historical context, were very likely persistent and that '…increasing departure from historical conditions logically requires a greater degree a greater degree of proof that a population is indeed viable' (Spence et al., 2008)" (DRP, p. 172). Given the scarcity of information on the historical populations and on many current populations, this is reasonable, except that the word "proof" seems inappropriate.[3] However, the DRP needs to describe the kind of evidence that is needed to overcome the "natural conditions" presumption. To see why, consider that the DRP (through Spence et al. 2008) relies on the literature for developing the connectivity criterion. This leads to the result that, in natural conditions, CC Chinook would not meet the connectivity criterion, since for topographic reasons there are no streams large enough to support Chinook for a considerable distance south from the Mattole River. Evidently, the natural conditions presumption can be overruled, since the DRP does so, so the DRP should describe the grounds on which this can be done.

Ocean harvest:

Ocean harvest of CCC Chinook is described in the DRP, but needs more attention. The DRP notes that data on the harvest rate of CCC Chinook are lacking, but uses the harvest rate of four year-old Klamath Chinook as an index. Given the lack of data, this is reasonable, but it would be useful to know how this relates to the estimated total harvest rate for Klamath Chinook, since the total harvest rate of CCC Chinook should be of interest. Moreover, any analysis of the effects of harvest at approximately the rate of the index on the CC Chinook is lacking. If, say, passage barriers were resulting in 10% mortality of adult CC Chinook, it seems likely that the DRP would make much of the issue, and recommend actions to address it. Simply saying that "Several ocean and freshwater fishing regulations have been modified to further conserve CC Chinook and other salmonids including gear restrictions, area and seasonal closures, and size limits," or noting that harvest levels have not recently increased, is not enough. The DPR needs some analysis of the effects of plausible harvest rates on the abundance and productivity of CC Chinook, and it should call for genetic analyses of samples from the ocean harvest to get some actual data on the harvest rate for CC Chinook. There should also be some analysis of the lasting effects of past harvest rates, which presumably were higher. For example, past harvest apparently has reduced the diversity in the age of maturity for Central Valley Chinook (Williams 2006), as it has for other exploited species (Sharpe and Hendry 2009). There are no data for CC Chinook, but there are data on Klamath River Chinook that could be used to assess this question. Similarly, what were the ecological effects of past harvest rates, in terms of reducing the subsidy of marine-derived nutrients to the watersheds? The analysis of Hill et al. (2010) suggests that this issue may matter.

[3] Math has proofs; science has evidence.

The DPR correctly notes that "Harvest impacts to Chinook salmon in this ESU occur primarily from ocean fisheries that are driven by hatchery production of Chinook salmon from outside the ESU (i.e. the Klamath basin and Central Valley)." This begs the question, what is NMFS going to do about this hatchery production? The problem that mixed-stock fisheries can result in an unsustainable level of harvest on the weaker stocks has long been known (e.g., Larkin 1977), but has also been long ignored. It is time to stop.

1.2 Where available, are opposing scientific studies or theories acknowledged and discussed?

Installation of large woody debris is a recurring recommendation in the DRP, but the considerable discussion of the effectiveness of the approach in the literature is not discussed in the DRP. A recent meta-analysis provides some support for the effectiveness of instream structures (Whiteway et al. 2010), but notes that "the scarcity of long-term monitoring of the effectiveness of in-stream structures is problematic." My understanding is that current standards for good practice call for careful consideration of geomorphic context is selecting sites for large wood installations, taking account of stream processes rather than just structure, and I do not see this reflected in the DRP.

The DRP could do a better job of reviewing the long-standing controversy regarding hatchery fish. The hatchery chapter in Williams (2006) discusses this controversy.

1.3 Are the scientific conclusions sound and derived logically from the results?

Recovery plans should be based on science, but are not themselves science, so they don't reach scientific conclusions. Therefore I am uncertain how to answer this question, except in terms of the basis for recommendations, or in terms of things like *IPkm*, which are discussed in other sections of this review.

2.0 Evaluate the recommendations made in the Report.

I did not find support for some of the recommendations in the DPR. In particular, other than the viability criteria that come from Spence et al. (2008), the de-listing criteria seem to come from nowhere. The numerical de-listing targets for dependent populations are not even given. The citation to CFR 17.11 and 17.12 does not help (these seem to be lists of listed wildlife and plants), and the only other citation in the relevant chapter (9) is to Spence et al. (2008). Spence et al. (2008) discuss the potential role of dependent populations in terms of metapopulation theory, but I did not find anything inking linking that discussion with the numerical targets for dependent populations in Vol. 1.

The DRP recommends that conservation hatcheries be considered for a number of populations. Such considerations should take account of the recent literature on the negative effects of hatchery fish on naturally reproducing fish noted in the response to question 1.1. The recommendations for adding large wood to streams have only weak support in the literature, as noted above. Generally, the recommendations do not provide for the kind of adaptive or experimental approach that is appropriate, given the scientific uncertainty associated with the recommendations. This is discussed further below. The DRP does recommend population monitoring with a modern sampling design, which is an important advance.

2.1 Does the plan meet the minimum standards for recovery plans outlined in the NMFS Interim Recovery Guidance and mandates described in section 4(f)(1)(b) of ESA to include site-specific management actions, objective measurable criteria (criteria that links to listing factors) and estimates of time and cost?

According to Sec. 2 of the ESA, "The purposes of this Act are to provide a means whereby the ecosystems upon which endangered species and threatened species depend may be conserved, to provide a program for the conservation of such endangered species and threatened species, …". This provides a logical basis for delisting criteria dealing with habitats, and not just abundance. The DR does this by using CAP ratings, but this makes the criteria opaque, and trying to work backwards through the rating scheme to the actual factors on the landscape that affect the species could be difficult. It may also be difficult to update the ratings as new understanding of habitat use by salmonids develops. One way to deal with this problem would be to write qualitative criteria, and then use the CAP ratings as the default way to make them quantitative.

The stream profiles in Volume 2 present the site-specific recommendations called for by Section 4(f)(1)(B) of the ESA. Unfortunately, many of these are works in progress. For example, the Implementation Table for Novato Creek is 14 pages of mostly boilerplate recommendations, with no costs, schedules, potential partners or comments given, and the table for the Petaluma River is similar. The tables for Pescadero, Scott and Soquel creeks are more filled in, but still seem incomplete. Many of the rows in the table, such as "Establish and/or maintain continuous riparian buffers" might better be covered in Ch. 8 of Vol. 1, unless site-specific recommendations are made. There are no tables, and so no site-specific actions, for streams lacking profiles.

The DPR lists several recovery actions to deal with harvesting and collecting generally, but some significant recovery action is needed to deal with effects of ocean harvest on CCC Chinook, even if it only involves data collection and modeling to develop a better understanding of the effects of ocean harvest on the ESU.

2.2 Are the results in the Report supported by the information presented?

In my view, recovery plans do not include results, as that term is normally used in science, so I am uncertain how to answer this question.

2.3 Does the recovery strategy and overall recovery plan provide clear guidance for the public, restorationists, managers, regulators and others to act in a relevant manner over the next several decades to promulgate recovery of salmon and steelhead?

I did not find Chapter 7 (Strategy for Recovery) to be informative; essentially, it passes the buck to the stream profiles. These seem better, but are of variable quality, and do not embody a properly adaptive approach, as discussed below. However, they do explain to other interested parties the sorts of things that the authors of the DPR would like to see happen.

2.4 Review the research and monitoring recommendations made in the Report and make any additional recommendations, if warranted.

Monitoring:

The monitoring chapter, incorporating an up-do-date sampling plan, is one of the high points in the DPR. However, given the lack of data on the harvest rate of CC Chinook are lacking, it seems logical that the monitoring program should include genetic analyses of tissue samples from the harvest to provide such data, as noted above. The abundance monitoring called for in the chapter would then allow for estimates of the harvest rate.

"Cause and effect" monitoring, as described in the 2007 guidelines, is also in order. Werner et al. (2005) discuss an approach that could be used for monitoring heat stress; see Ch. 15 in Williams (2006) and Williams et al. (2007) for other examples. Generally, cause and effect monitoring is needed for an adaptive approach, as discussed below.

There is a need for monitoring to assess the effectiveness of large wood installations, as noted by Whiteway et al. 2010. The DRP should also recommend some quantitative assessment of how much large wood might have to be placed in streams to cause a given increase in abundance.

Additional Recommendations

As noted above, the DRP needs major revision, both in style and substance. Regarding style, the recovery effort has a much greater chance of success if those interested in or affected by it can understand what the DRP says. Some sections of the DRP are clear and well written; examples are Ch. 1 in Vol. 1 and appendices A and G in Vol. 3. Many other sections need improvement, in some cases a lot. Generally, using plain language and the active voice, avoiding jargon and organization-speak, and omitting needless words will contribute to the utility of the DPR. In the current draft, it is easy to get confused in the parallel and repetitive treatments of the three listed ESU/DPSs. If these can be partly combined, it would be helpful. More attention should be given to citations. Citations serve various purposes, but in scientific writing the main purpose is to answer the question that readers should always ask: "why should I believe this?" With a citation, the writer is implicitly making the statement: "This gives the reason(s) why I believe what I just wrote." In other words, the citation is supporting an argument, not establishing a fact, so using good citations is important for making the document persuasive. Citations, even to the peer-reviewed literature, are not like theorems in mathematics, and, except for perhaps citations to the Federal Register about the dates of particular regulatory actions, do not establish validity.

Recent publications

The DRP should be revised to take account of scientific developments since the TRT completed its work, as discussed above. Examples include the recent publications on hatcheries and steelhead noted above, and the 2011 NMFS status review of listed salmon and steelhead.[4] Using Google Scholar to check who has cited salient papers during this period makes finding recent papers easy, so this task is not as difficult as it may appear at first glance.

Physical habitats

NMFS should consider adding a chapter describing the diverse physical habitats in the domain in the DRP. I think this would facilitate making the other chapters more readable. The chapter could include plates showing the topography and the distributions of annual rainfall, dominant vegetation type, summer temperature, etc. Ch. 3 in Williams (2006) exemplifies what I have in mind, and its Appendix A may provide other ideas, such as including area-elevation plots and discharge plots in the stream profiles. Generally, the stream profiles would be improved by short descriptions of the physical geography of the basins, to go with the descriptions of land use history, etc.

Adaptive management

Given the scarcity of data on the ESU/DPSs in question, the DRP should incorporate an adaptive approach, along the lines discussed in the 2007 guidelines. The definition of adaptive management given in the glossary of Vol. 1 does not seem consistent with the guidelines, and I do not see an adaptive approach in the DRP. One simple way to think about adaptive management is to think of it as experimental management. Given the uncertainty involved, management is going to be experimental, whether this is intended or not. The trick of adaptive management is to make the experiments informative. For various reasons, classical 'active' adaptive management is very difficult to do except on a small geographic scale, but the strong year to year variation in driving variables such as flow should allow for effective learning (Williams 1998), especially if 'cause and effect' monitoring is included in the monitoring plan.

It is also useful to articulate the ideas that motivate restoration actions, and consider how these can be tested. For example, the life-stage targets for Chinook in the DPR seem to embody the idea that the most important juvenile life history trajectory involves a few months of

[4] This is a draft with a header saying "Do Not Cite or Distribute," but it is available on the Internet, and is cited by the five-year review of Central Valley Winter Chinook.

residence in or near the spawning areas. However, in streams with good estuaries, the fry migrant life history may be more important, as seems to be the case for fall Chinook in the Central Valley (Williams 2006; in press; Miller et al. 2010). For example, the Eel River historically had extensive tidal habitat around the estuary, and this habitat may have been critical for the population there, which was large enough that hatcheries were built to exploit it. Data to address this issue could be obtained by collecting otoliths during carcass surveys, and conducting microstructural and microchemical analyses, as in Miller et al. (2010).

Probably it is too late for this plan, but NMFS should consider using Bayesian Networks (BNs) in future plans, or in updates of this one. Bayesian Networks have been used successfully for environmental management in various situations, and seem well suited for stakeholder processes (e.g., Marcot et al. 2001, 2006; Steventon 2008; Hart and Polino 2009). They are also well suited for adaptive management, since they can include proposed management action, and account explicitly for uncertainty. Steventon (2008) describes an application very similar to recovery planning, and the appendix to Hart and Polino (2009) gives a good introduction to BNs generally.

Marijuana production

The DPR does mention *cannabis* production, but does not give it enough attention. Marijuana production is a major industry in the northern part of the domain. This has resulted in a sharply increased human population in remote areas of the domain, many miles of dirt road that may be used year-round, construction of many greenhouse pads, increased summer diversions from headwater streams, etc. Full legalization of marijuana production, which would result in production moving to areas with lower costs, would be a large step toward recovery for a significant number of salmonid populations.

Concluding Remarks

Since my review of the DRP is largely negative, I want to emphasize how very difficult it is to meet the standards set by the ESA, especially because writing recovery plans is assigned to the regulatory rather than the scientific branch of NMFS. People working for the regulatory branch have more than enough to do in that function, and simply do not have the time to keep up with scientific developments. Moreover, regulation calls for a different mind-set than science does; that is, biologists working for the regulatory branch generally have to practice what is sometimes called “combat biology.” This is an honorable task, on which protection of the environment too often depends, but it is not science. Staff from the regulatory branch need to be deeply involved with writing recovery plans, since it will fall on them to implement the plans, and the plans need to take more than scientific considerations into account, but practicing scientists need to be involved as well.

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Appendix A

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Appendix B

Statement of Work for Dr. John Williams

California's North Central Coast Domain Draft Recovery Plan

Scope of Work and CIE Process: The National Marine Fisheries Service's (NMFS) Office of Science and Technology coordinates and manages a contract providing external expertise through the Center for Independent Experts (CIE) to conduct independent peer reviews of NMFS scientific projects. The Statement of Work (SoW) described herein was established by the NMFS Project Contact and Contracting Officer's Technical Representative (COTR), and reviewed by CIE for compliance with their policy for providing independent expertise that can provide impartial and independent peer review without conflicts of interest. CIE reviewers are selected by the CIE Steering Committee and CIE Coordination Team to conduct the independent peer review of NMFS science in compliance the predetermined Terms of Reference (ToRs) of the peer review. Each CIE reviewer is contracted to deliver an independent peer review report to be approved by the CIE Steering Committee and the report is to be formatted with content requirements as specified in Annex 1. This SoW describes the work tasks and deliverables of the CIE reviewer for conducting an independent peer review of the following NMFS project. Further information on the CIE process can be obtained from www.ciereviews.org.

Project Background: The Endangered Species Act (ESA) requires NOAA's National Marine Fisheries Service (NMFS) develop and implement recovery plans for the conservation of threatened and endangered species. The North Central Coast Draft Recovery Plan will include three ESA-listed populations: (1) Northern California steelhead (threatened); (2) California Coastal Chinook salmon (threatened); (3) Central California Coast steelhead (threatened). The draft recovery plan serves as a guideline for achieving recovery goals by describing the steps that must be taken to improve the status of the species and their habitats. Although the recovery plan itself is not a regulatory document, its primary purpose is to provide a conservation "road map" for Federal and state agencies, local governments, non-governmental entities, private businesses, and stakeholders. The NMFS Recovery Plan for the North Central Coast is expected to generate substantial interest from outside parties because it: (1) will contain recommendations involving water supplies for a variety of municipalities (including the greater San Francisco area) and agricultural users; (2) will prioritize watersheds for targeted restoration actions; (3) could influence local and regional planning efforts and decisions involving land development patterns such as county policies and forest practices; and (4) may advise state agencies and local governments on other actions necessary for recovery. The draft recovery plan will include a large geographic area in central California and has the potential for wide-ranging implications. Stakeholder interest will be high and likely lead to inquiries from elected representatives at the state and Federal levels.

Requirements for CIE Reviewers: Three CIE reviewers shall conduct an impartial and independent peer review in accordance with the SoW and ToRs herein. The CIE reviewers shall conduct the peer review as a 'desk' review (i.e., the review and report writing can be accomplished from their primary locations, therefore no travel is required). Each reviewer's duties shall not exceed a maximum of ten work days.

CIE reviewers shall have expertise in salmon management, salmon conservation biology, salmon restoration practices, and salmon/water management, and it is desirable that reviewers have experience in salmon conservation under the ESA and strong credentials in west coast salmon management activities. The CIE reviewers shall have the requested expertise necessary to complete an impartial peer review and produce the deliverables in accordance with the SoW and the ToRs as stated herein (refer to the ToR in Annex 1).

The CIE reviewers shall conduct a 'desk' peer review of the California's North Central Coast Domain Draft Recovery Plan Report to ensure that its contents can be factually supported and that the methodology and conclusions are scientifically valid. The area under consideration will be the lands and waterways in Northern and Central California. Each reviewer shall conduct the peer review and develop a detailed report addressing each of the ToRs as specified in Annex 1.

Statement of Tasks for CIE Reviewers: The CIE reviewers shall conduct necessary preparations prior to the peer review, conduct the peer review, and complete the deliverables in accordance with the ToR and milestone dates as specified in the Schedule section.

Prior to the Peer Review: Upon completion of the CIE reviewer selection by the CIE Steering Committee, the CIE shall provide the CIE reviewer information (full name, title, affiliation, country, address, email) to the COTR, who forwards this information to the NMFS Project Contact no later the date specified in the Schedule of Milestones and Deliverables. The CIE is responsible for providing the SoW and ToRs to the CIE reviewers. The NMFS Project Contact is responsible for providing the CIE reviewers with the background documents, reports, and other pertinent information. Any changes to the SoW or ToRs must be made through the COTR prior to the commencement of the peer review.

Pre-review Background Documents: Two weeks before the peer review, the NMFS Project Contact will send (by electronic mail or make available at an FTP site) to the CIE reviewers the necessary background information and reports for the peer review. In the case where the documents need to be mailed, the NMFS Project Contact will consult with the CIE Lead Coordinator on where to send documents. CIE reviewers are responsible only for the pre-review documents that are delivered to the reviewer in accordance to the SoW scheduled deadlines specified herein. The CIE reviewers shall read all documents in preparation for the peer review.

Desk Peer Review: Each CIE reviewer shall conduct the independent peer review from their primary locations as a "desk" review in accordance with the SoW and ToRs to ensure the best available science is utilized for the National Marine Fisheries Service (NMFS) management decisions (refer to the ToR in Annex 1). Modifications to the SoW and ToRs can not be made during the peer review, and any SoW or ToRs modifications prior to the peer review shall be approved by the COTR and CIE Lead Coordinator. The CIE Lead Coordinator can contact the Project Contact to confirm any peer review arrangements.

Contract Deliverables - Independent CIE Peer Review Reports: Each CIE reviewer shall complete an independent peer review report in accordance with the SoW. Each CIE reviewer shall complete the independent peer review according to required format and content as described in Annex 1. Each CIE reviewer shall complete the independent peer review addressing each ToR as described in Annex 2.

Specific Tasks for CIE Reviewers: The following chronological list of tasks shall be completed by each CIE reviewer in a timely manner as specified in the **Schedule of Milestones and Deliverables**.

1) Conduct necessary pre-review preparations, including the review of background material and reports provided by the NMFS Project Contact in advance of the peer review.
2) Conduct an independent peer review in accordance with the ToRs (**Annex 2**).
3) No later than November 14, 2011, each CIE reviewer shall submit an independent peer review report addressed to the "Center for Independent Experts," and sent to Mr. Manoj Shivlani, CIE Lead Coordinator, via email to shivlanim@bellsouth.net, and CIE Regional Coordinator, via email to Dr. David Die ddie@rsmas.miami.edu. Each CIE report shall be written using the format and content requirements specified in Annex 1, and address each ToR in **Annex 2**.

Schedule of Milestones and Deliverables: CIE shall complete the tasks and deliverables described in this SoW in accordance with the following schedule.
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28 October 2011	CIE shall provide the COTR with the CIE reviewer contact information, which will then be sent to the Project Contact
28 October 2011	The Project Contact will send the CIE Reviewers the report and background documents
31 October – 14 November 2011	Each reviewer shall conduct an independent peer review
14 November	Each reviewer shall submit an independent peer review report to the CIE
28 November 2011	CIE shall submit draft CIE independent peer review reports to the COTRs
5 December 2011	The COTRs will distribute the final CIE reports to the Project Contact

Modifications to the Statement of Work: This 'Time and Materials' task order may require an update or modification due to possible changes to the terms of reference or schedule of milestones resulting from the fishery management decision process of the NOAA Leadership, Fishery Management Council, and Council's SSC advisory committee. A request to modify this SoW must be approved by the Contracting Officer at least 15 working days prior to making any permanent changes. The Contracting Officer will notify the COTR within 10 working days after receipt of all required information of the decision on changes. The COTR can approve changes to the milestone dates, list of pre-review documents, and ToRs within the SoW as long as the role and ability of the CIE reviewers to complete the deliverable in accordance with the SoW is not adversely impacted. The SoW and ToRs shall not be changed once the peer review has begun.

Acceptance of Deliverables: Upon review and acceptance of the CIE independent peer review reports by the CIE Lead Coordinator, Regional Coordinator, and Steering Committee, these reports shall be sent to the COTR for final approval as contract deliverables based on compliance with the SoW and ToRs. As specified in the Schedule of Milestones and Deliverables, the CIE shall send via e-mail the contract deliverables (CIE independent peer review reports) to the COTR (William Michaels, via William.Michaels@noaa.gov).

Modifications to the Statement of Work: This 'Time and Materials' task order may require an update or modification due to possible changes to the terms of reference or schedule of milestones resulting from the fishery management decision process of the NOAA Leadership, Fishery Management Council, and Council's SSC advisory committee. A request to modify this SoW must be approved by the Contracting Officer at least 15 working days prior to making any permanent changes. The Contracting Officer will notify the COTR within 10 working days after receipt of all required information of the decision on changes. The COTR can approve changes to the milestone dates, list of pre-review documents, and ToRs within the SoW as long as the role and ability of the CIE reviewers to complete the deliverable in accordance with the SoW is not adversely impacted. The SoW and ToRs shall not be changed once the peer review has begun.

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Support Personnel:

William Michaels, Program Manager, COTR
NMFS Office of Science and Technology
1315 East West Hwy, SSMC3, F/ST4, Silver Spring, MD 20910
William.Michaels@noaa.gov Phone: 301-427-8155

Manoj Shivlani, CIE Lead Coordinator
Northern Taiga Ventures, Inc.
10600 SW 131st Court, Miami, FL 33186
shivlanim@bellsouth.net Phone: 305-383-4229

Roger W. Peretti, Executive Vice President
Northern Taiga Ventures, Inc. (NTVI)
22375 Broderick Drive, Suite 215, Sterling, VA 20166
RPerretti@ntvifederal.com Phone: 571-223-7717

Key Personnel:

NMFS Project Contact:
Dick Butler
NMFS, Santa Rosa Area Office Supervisor
777 Sonoma Ave, Rm 325, Santa Rosa, CA 95404-6515
Dick.Butler@noaa.gov Phone: 707-575-6058

ANNEX 1:

Terms of References (ToRs)

CIE Peer Review of

California's North Central Coast Domain Draft Recovery Plan

The scope of work should focus on the principal elements required in a recovery plan. These principal elements have been defined in section 4(f)(1) of the federal Endangered Species Act (ESA) and sections 1.1 and 1.2 of the National Marine Fisheries Service Interim Recovery Planning Guidance (NMFS 2006)

Section 4(f)(1)(b) of ESA states that "each plan must include, to the maximum extent practicable,

- a description of such site-specific management actions as may be necessary to achieve the plan's goal for the conservation and survival of the species;
- objective, measurable criteria which, when met, would result in a determination…that the species be removed from the list; and,
- estimates of the time required and the cost to carry out those measures needed to achieve the plan's goal and to achieve intermediate steps toward that goal."

From section 1.1 of NMFS (2006), a recovery plan should:

- "Delineate those aspects of the species' biology, life history, and threats that are pertinent to its endangerment and recovery;
- Outline and justify a strategy to achieve recovery;
- Identify the actions necessary to achieve recovery of the species; and
- Identify goals and criteria by which to measure the species' achievement of recovery."

Background Materials Required
There are two NMFS Science Center Technical Memoranda that form the biological framework for the recovery plan: historical population structure and viability criteria. These memoranda and other supporting information are critical to the review of the Draft NCCC Recovery Plan and include:

- Technical Recovery Team Reports: Historical Structure
  - http://swfsc.noaa.gov/publications/FED/00671.pdf
- Technical Recovery Team Framework for Assessing Viability
  - http://swfsc.noaa.gov/publications/FED/00885.pdf
- 2006 (2007 Updates) NMFS Interim Recovery Planning Guidance
  - http://www.nmfs.noaa.gov/pr/recovery/
- Endangered Species Act (http://www.nmfs.noaa.gov/pr/pdfs/laws/esa.pdf)

ANNEX 1 (continued):

Terms of References (ToRs)

CIE Peer Review of
California's North Central Coast Domain Draft Recovery Plan

1.0 Evaluate the adequacy, appropriateness and application of data used in the Report.

1.1 In general, does the Report include and cite the best scientific and commercial information available on the species and its habitats, including threats to the species and to its habitat including large-scale perturbations such as climate change and ocean conditions?

1.2 Where available, are opposing scientific studies or theories acknowledged and discussed?

1.3 Are the scientific conclusions sound and derived logically from the results?

2.0 Evaluate the recommendations made in the Report.

2.1 Does the plan meet the minimum standards for recovery plans outlined in the NMFS Interim Recovery Guidance and mandates described in section 4(f)(1)(b) of ESA to include site-specific management actions, objective measurable criteria (criteria that links to listing factors) and estimates of time and cost?

2.2 Are the results in the Report supported by the information presented?

2.3 Does the recovery strategy and overall recovery plan provide clear guidance for the public, restorationists, managers, regulators and others to act in a relevant manner over the next several decades to promulgate recovery of salmon and steelhead?

2.4 Review the research and monitoring recommendations made in the Report and make any additional recommendations, if warranted.

CIE reviewers are contracted with the qualifications to conduct a scientific peer review, and are not required to provide regulatory or management advice.

ANNEX 2

Format and Contents of CIE Independent Reports

The report should follow the outline given below. It should be prefaced with an Executive Summary that is a concise synopsis of goals for the peer review, findings, conclusions, and recommendations. The main body of the report should provide an introduction that includes a background on the purpose of the review, the terms of reference and a description of the activities the reviewer took while conducting the review. Next, the report should include a summary of findings made in the peer review followed by a section of conclusions and recommendations based on the terms of reference. Lastly the report should include appendices of information used in the review (see outline for more details).

1. Executive Summary
   a. Impetus and goals for the review
   b. Main conclusions and recommendations
   c. Interpretation of the findings with respect to conclusions and management advice

2. Introduction
   a. Background
   b. Terms of Reference
   c. Description of activities in the review

3. Review of Information used in the Status Review Report (as outlined in the table of contents in the Status Review Report)

4. Review of the Findings made in the Status Review Report
   a. DPS/ESU Considerations: Populations-Habitats-Threats
   b. Extinction Risk Analysis and Recovery Criteria
   c. Evaluation of Regulatory and Non-regulatory Recovery Actions
   d. Research and Monitoring Recommendations

5. Summary of findings made by the CIE peer reviewer

6. Conclusions and Recommendations (based on the Terms of Reference in Annex I)

7. Appendices
   a. Bibliography of all material provided
   b. Statement of Work
   c. Other

Appendix C
Abstracts of selected recent articles regarding hatchery fish

Araki et al. 2007:
Captive breeding is used to supplement populations of many species that are declining in the wild. The suitability of and long-term species survival from such programs remain largely untested, however. We measured lifetime reproductive success of the first two generations of steelhead trout that were reared in captivity and bred in the wild after they were released. By reconstructing a three-generation pedigree with microsatellite markers, we show that genetic effects of domestication reduce subsequent reproductive capabilities by ~40% per captive-reared generation when fish are moved to natural environments. These results suggest that even a few generations of domestication may have negative effects on natural reproduction in the wild and that the repeated use of captive-reared parents to supplement wild populations should be carefully reconsidered.

Araki et al. 2008:
Accumulating data indicate that hatchery fish have lower fitness in natural environments than wild fish. This fitness decline can occur very quickly, sometimes following only one or two generations of captive rearing. In this review, we summarize existing data on the fitness of hatchery fish in the wild, and we investigate the conditions under which rapid fitness declines can occur. The summary of studies to date suggests: nonlocal hatchery stocks consistently reproduce very poorly in the wild; hatchery stocks that use wild, local fish for captive propagation generally perform better than nonlocal stocks, but often worse than wild fish. However, the data above are from a limited number of studies and species, and more studies are needed before one can generalize further. We used a simple quantitative genetic model to evaluate whether domestication selection is a sufficient explanation for some observed rapid fitness declines. We show that if selection acts on a single trait, such rapid effects can be explained only when selection is very strong, both in captivity and in the wild, and when the heritability of the trait under selection is high. If selection acts on multiple traits throughout the life cycle, rapid fitness declines are plausible.

Araki et al. 2009:
Supplementation of wild populations with captive-bred organisms is a common practice for conservation of threatened wild populations. Yet it is largely unknown whether such programmes actually help population size recovery. While a negative genetic effect of captive breeding that decreases fitness of captive-bred organisms has been detected, there is no direct evidence for a carry-over effect of captive breeding in their wild-born descendants, which would drag down the fitness of the wild population in subsequent generations. In this study, we use genetic parentage assignments to reconstruct a pedigree and estimate reproductive fitness of the

wild-born descendants of captive-bred parents in a supplemented population of steelhead trout (Oncorhynchus mykiss). The estimated fitness varied among years, but overall relative reproductive fitness was only 37 per cent in wild-born fish from two captive-bred parents and 87 per cent in those from one captive-bred and one wild parent (relative to those from two wild parents). Our results suggest a significant carry-over effect of captive breeding, which has negative influence on the size of the wild population in the generation after supplementation. In this population, the population fitness could have been 8 per cent higher if there was no carry-over effect during the study period.

Berejikian et al. (2009):
Estimates of the relative fitness of hatchery- and natural-origin salmon can help determine the value of hatchery stocks in contributing to recovery efforts. This study compared the adult to fry reproductive success of natural-origin summer chum salmon (*Oncorhynchus keta*) with that of first- to third-generation hatchery-origin salmon in an experiment that included four replicate breeding groups. Hatchery- and natural-origin chum salmon exhibited similar reproductive success. Hatchery- and natural-origin males obtained similar access to nesting females, and females of both types exhibited similar breeding behaviors and durations. Male body size was positively correlated with access to nesting females and reproductive success. The estimates of relative reproductive success (hatchery/natural = 0.83) in this study were similar to those in other studies of other anadromous salmonids in which the hatchery population was founded from the local natural population and much higher than those in studies that evaluated the lifetime relative reproductive success of nonlocal hatchery populations.

Bernston et al. 2011:
Hatchery supplementation programs are designed to enhance natural production and maintain the fitness of the target population; however, it can be difficult to evaluate the success of these programs. Key to the success of such programs is a relatively high reproductive success of hatchery fish. This study investigated the relative reproductive success (RRS) of steelhead *Oncorhynchus mykiss* (anadromous rainbow trout) by creating pedigrees for hatchery and natural spawning steelhead. We genotyped adult steelhead that returned to a weir and were released upstream to spawn in Little Sheep Creek, a tributary to the Imnaha River in eastern Oregon. The broodstock for this supplementation program were originally chosen from natural-origin steelhead returning to the weir and in subsequent years consisted of both natural- and hatchery-origin individuals. Microsatellite analyses showed the broodstock to be genetically similar to the natural population across years. We also genotyped adult resident rainbow trout from multiple locations upstream of the weir and determined the parentage of progeny collected at various life history stages, including returning adults in subsequent years. Analysis of progeny sampled at both the juvenile and adult life stages suggested that the RRS of hatchery-origin fish was 30–60% that of their natural-origin counterparts. Using generalized linear models to address the importance of various factors associated with reduced reproductive success, we found that the

greatest effects on RRS were origin (natural versus hatchery), length, return date, and the number of same-sex competitors. Natural parents were less negatively affected by same-sex competitors. Differential survival of juveniles and the behavior of offspring and/or spawning adults may all contribute to diminished fitness in hatchery-reared salmon, although it could not be determined to what extent these effects were of a persistent, heritable nature as distinct from an environmental effect associated with hatchery rearing and release strategies.

Buhle et al. 2009:
Efforts to conserve depleted populations of Pacific salmon (Oncorhynchus spp.) often rely on hatchery programs to offset losses of fish from natural and anthropogenic causes, but their use has been contentious. We examined the impact of a large-scale reduction in hatchery stocking on 15 populations of wild coho salmon along the coast of Oregon (USA). Our analyses highlight four critical factors influencing the productivity of these populations: (1) negative density-dependent effects of hatchery-origin spawners were _5 times greater than those of wild spawners; (2) the productivity of wild salmon decreased as releases of hatchery juveniles increased; (3) salmon production was positively related to an index of freshwater habitat quality; and (4) ocean conditions strongly affect productivity at large spatial scales, potentially masking more localized drivers. These results suggest that hatchery programs' unintended negative effects on wild salmon populations, and their role in salmon recovery, should be considered in the context of other ecological drivers.

Chilcote et al. 2011.
We found a negative relationship between the reproductive performance in natural, anadromous populations of steelhead trout (*Oncorhynchus mykiss*), coho salmon (*O. kisutch*), and Chinook salmon (*O. tshawytscha*), and the proportion of hatchery fish in the spawning population. We used intrinsic productivity as estimated from fitting a variety of recruitment models to abundance data for each population as our indicator of reproductive performance. The magnitude of this negative relationship is such that we predict the recruitment performance for a population composed entirely of hatchery fish would be 0.128 of that for a population composed entirely of wild fish. The effect of hatchery fish on reproductive performance was the same among all three species. Further, the impact of hatchery fish from ''wild type'' hatchery broodstocks was no less adverse than hatchery fish from traditional, domesticated broodstocks. We also found no support for the hypothesis that a population's reproductive performance was affected by the length of exposure to hatchery fish. In most cases, measures that minimize the interactions between wild and hatchery fish will be the best long-term conservation strategy for wild populations.